

Anthropology is companion studies: a study of violent relations during fieldwork with my family

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Anthropology is Companion Studies: A study of violent relations during fieldwork with my family

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Abstract:	<p>Companions in the field influence ethnography, so affecting research position whilst providing research insights. The research of violence takes place within the relations of violence being studied. Family companions who used to live in a field site are entwined in those violent relations. Capacity for violent threat or suffering is connected to the companions of family. Reporting on companions is problematic. Where anonymity is compromised, key information has to be withheld, which in turn, leads readers to question integrity. The ethical balance between author, companions and reader reveals more about the relationality of violence. As anthropology turns towards analysing a relational positioning between multifarious companions, the study of family companions expands to consider how intimate companions, of one kind or another, unavoidably shape ethnography. The paper contributes to understanding violence, the research of violence, accompanied fieldwork and methodology in relational anthropological frameworks.</p>
<p>Note: The following files were submitted by the author for peer review, but cannot be converted to PDF. You must view these files (e.g. movies) online.</p>	
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3 Introduction
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7 Ethnography is formed through reflexive immersion in the relations that emerge
8 between an ethnographer and their various companions. Through ethnography,
9 anthropology reveals ‘relational intimacy’ which is explained using ethics and
10 reflection (Noah, 2018: 306). In ‘relational anthropology’ (Noah, 2018; Pedersen,
11 2012), companions, of one kind or another (which could include humans, other
12 species, environments, materials and much more), shape and are shaped by other
13 companions. This study of companions in fieldwork adopts a relational view of
14 violence and the research of violence. During field research accompanied by my
15 family, I became aware of violent relations whose constitution included my family. I
16 use my encounter with my intimate relations to explore violence, problems
17 researching violence and the effect of unavoidable companions on an anthropology
18 derived from intimate relations.
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29 First, I show that despite efforts to exclude my family companions from my research,
30 family relations provided insight into how conversations about neighbours and
31 relatives situate violence and (in)security. Second, I review literature on
32 accompanied fieldwork which, problematically, encourages disclosure about access,
33 too easily identifies the position of companions, and offers narrow interpretations of
34 companions. Third, I use the ethnography to question whether authors of
35 ethnography, and their intimate companions, can be afforded appropriate privacy
36 when readers need to know something of the private about them. Finally, I show that
37 the idea of companion fieldwork as a methodological niche is anachronistic. In a
38 more-than-human sociality (Tsing, 2013), ethnographers are surrounded by an
39 abundance of intimate companions that do not actively engage in research but,
40 nevertheless, provide insights and situate the research.
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51 Inspired by companions who are fieldwork research assistants, Gupta moves beyond
52 his earlier thinking on temporal and spatial field relations to propose ‘the field’ as a
53 ‘network of relationships...constantly moving’ (Gupta, 2014: 399). The formulation of
54 a relational field and the methodological inquiries into fieldwork companions are,
55 nevertheless, disconnected. The primary purpose of most discussion on
56 ‘accompanied fieldwork’ is to push companions out from the shadows of research.
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Most companions are identified as human research assistants or family - this paper is no exception. The discussion starts by looking at how family companions are entangled in ethnographic research but the paper finishes with a view of ethnography saturated by companions being brought together by emerging and practised, fluid relations. Here companions, of one kind or another, shape and are shaped, by other companions. Thus, the discussion on relational violence frames the narrative on the methodological and analytical intertwine of a relational anthropology.

The ethnography in this paper focuses on my family - Colombian migrants to the UK, who returned to their home region in Colombia while I conducted research on coffee farming and violence. Through my family, I began to see how daily conversation, at least as much as silence, shaped interpretations of violence based on who people knew and what they knew about them. Our position in the field included our potential to be affected by, or potential to instigate, violence. In these circumstances, the anthropological priority to report on an author's research position is ethically problematic.

The unvoiced contrast between what I am prepared to divulge about my companions in order to support scholarly argument and what the reader feels I should divulge is a key point. I do not justify through detailed argument my inclusion and exclusion of pieces of information about my family in this text. The presence or absence of family-related information was mediated by my expectations of what would be acceptable input for a critical readership. Yet it was also mediated by my selective compliance and, importantly, my refusal to provide that information. Any resulting ambiguity or narrative weakness serves to highlight the problematic, which is ultimately located at those points where integrity of argument is an ethical struggle between author and reader. Therefore, the intention is to provoke discussion that distinguishes any critique about my argument from what a reader can ethically ask their author to provide. I hope that the explicit limiting of information also offers readers a sense of who my family are in a field site where, in the midst of violence, information although plentiful, is partial and positioned.

This discussion is written for two main readership groups - critically engaged academics and my non-academic family who read how I represent them. Many

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scholars remind us that those who are studied can critically examine what is written about them (e.g. Brettell, 1993; Gordon, 1998; Mosse, 2006). My family skip over large parts, preferring ethnographic description to analysis. They trust my approach in this paper (or, at least, they tolerate my explanation of my approach). They know that I deliberately avoid too much divulgence about them, even though this tactic might encourage the reader to (over) imagine the possibilities of who we are.

I focus on the inevitable influence of companions rather than examining methodological techniques to avoid companion influence. Cautious that the omission of companions from text can help bolster the performance of the authoritative anthropologist (Gupta, 2014), I hope that the inclusion of insights from my companions demonstrate my naivety. I have kept the description of a murder to a minimum. For readers who want a more detailed account of homicide, there are plenty of descriptions out there.

Encountering Violent Positions

My doctoral fieldwork was my first practice of ethnographic research. It took place in the coffee fields of Colombia between 2010 and 2012. My wife, Sonia,¹ and two step-daughters (Isa and Julianna), grew up in these coffee hills before coming to the UK in 2000 when the children were 10 and 13 years old. I met them in 2003. Sonia and I have another daughter, Maria, born in 2007. As I travelled to Colombia to study the impact of violence on coffee farming, in their eyes they were travelling back home to live with their family and friends. England would always be a foreign place to live but they soon discovered that their relationship with Colombia had changed too. During our time there, Sonia said, ‘We migrants can never return home. Home is gone’. Sonia, Isa and Julianna came from Porcuna, a village that had been producing coffee since the 1970s. A few close relations had small coffee farms under three hectares while more distant relations had larger coffee farms over 50 hectares. Meeting Sonia’s family on an earlier visit to Colombia in 2006, had inspired the research topic, yet Sonia and her daughters had little interest in my research.

During fieldwork planning, I rejected researching my own family to avoid

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3 methodological problems of 'contaminated data' and 'research bias'. The potential
4 interrogation of my own family's dynamics also posed ethical problems. Research, I
5 had decided, would be confined to encounters outside of my family, while 'in the
6 field'. I thought I could leave the house and engage in a reflexive ethnographic
7 process in order to co-produce knowledge with the local population, while leaving
8 Sonia and the girls to engage in the village life that they had wanted to experience
9 again.

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12 To emphasise this detachment, I eventually lived in a different village called Andujar.
13 This move took place after a friend's sister invited me to judge a beauty contest in
14 the village school, which led to my introduction and subsequent access to a village
15 without any of our relatives. Andujar was a two-hour journey from Porcuna - far, but
16 not too far. We could still maintain the extended family life that Sonia and her
17 daughters wanted, so they agreed to live with me in this new base. I assumed that
18 geographic distance and kinship distance from the family in Porcuna would create a
19 research distance. Distance equated to reduced interference and thereby greater
20 methodological integrity: an uninterrupted view of a pure location. Yet despite my
21 best intentions, the juxtaposition of who we were in the two villages became hard to
22 ignore. The implications of a relational field (Gupta 2014) are easy to forget in the
23 plans to separate private life from public life.

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26 My view of violence soon arrived, and closer to home than I had anticipated. My
27 step-daughter, Isa, was drinking with an aunt in Porcuna when the aunt got into an
28 argument with a girl who was a similar age to Isa. The girl threatened to kill Isa. The
29 aunt responded by stabbing the girl in the back of the thigh (I was told that, locally,
30 this was seen as a non-life-threatening way of stabbing someone), and they carried
31 on drinking. The girl came back ten minutes later and stabbed the aunt several times
32 in the chest and she was rushed to hospital. Family discussions with neighbours
33 revealed how the girl was 'mala' (bad) but nothing could be done because her
34 boyfriend was a policeman. Within 24 hours the girl had left the village.

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37 Three months later, Isa got a job at the information desk in a shopping centre in
38 Medellin. One day she received a serious death threat while at work. The man knew
39 her telephone number, where she lived and how she got to work. He told her what

he would do with her dead body. This incident took place in a café at the shopping centre and during the aggressive rant the man put his gun-shaped hand to her forehead. The security guard at the shopping centre, thinking this man was her boyfriend, ignored the confrontation. From what we discovered later, the man was running a criminal scheme using young girls and a plastic surgeon; Isa had refused his offers of free surgery or other gifts. Sonia asked me, 'What are we going to do?'

After the initial drama, no one in the family looked particularly worried, apart from me; the conversation soon progressed from mortal threat to hair styling. Isa just wanted to go out with her friends as normal. When pressed, Sonia and Juliana said that Isa was really stupid, or up to no good, otherwise she would not have been threatened. Over the next couple of days we discussed sending Isa back to the UK, but settled with the wider family consensus to send her to Porcuna for a few weeks instead.

These incidents set a scene of violence, law and security. In a village, people can stab others in a bar then carry on drinking. Some people are 'bad'. Police cannot be trusted. Men can threaten women in public without intervention. The victim is to blame, either through stupidity or because they were up to no good. The perpetrator is left alone and not investigated. Removal (either as displacement or social cleansing) gets rid of violent risk. The experience would have been a good example of ethnographic evidence on how people navigated violence, except I was studying coffee farmers not my companions: ethnography had delivered the right parcel to the wrong address.

My understanding of our family was changing. For Isa's safety, we put her in a village which had, on average, one murder every two weeks. In Porcuna, lots of people knew Isa and our family, that was security; a type of security that meant, directly or indirectly, my family and their associates could provide some protection against potential killers. My separation between family life and research, nevertheless, continued - it was the ethical and methodological path we had all agreed upon, however, I decided to pay more attention to my changeable position.

Insider-outsider positioning derives from the interpretations of relative positions

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3 during reflexive, ethnographic experience. The concept is formed by the juxtaposition
4 of different passages of life and travel. These segments of personal lived mobility
5 can become a form of multi-sited ethnography. My own family had moved back to
6 their home, but it was not my home and, as mentioned earlier, not quite their home
7 either. Once we moved to Andujar, it was even less their home. The fieldwork was
8 conducted in their region, but not in their village. To understand our position using an
9 insider-outsider frame requires slightly convoluted unpacking.
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17 I was an obvious outsider, except that as a member of an inside family I was
18 perceived as more insider than if I had arrived with only professional connections of
19 access. My immediate family were insiders but they had lived outside and Sonia had
20 married an Englishman (and he was from a university!). For other villagers this
21 meant my immediate family had almost climbed a social class. Some of the richer
22 people in the village, who barely talked to Sonia before, now chatted to her as if they
23 were old friends, although they still maintained their usual stance to the rest of her
24 family. In comparison to Porcuna, we lived as outsiders in Andujar, yet as Sonia
25 shared a similar history to them she was also an insider. Commonalities and
26 differences between Sonia and locals in Andujar over-lapped and diverged,
27 emphasising the multi-vocal (Moore, 2007) or intersectional (Carbado et al., 2013)
28 aspects of Sonia and the people she met. These points of juncture between me,
29 Sonia and local life would create key moments of ethnographic significance in my
30 research on coffee and violence.
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43 In Andujar, when people first met Sonia, they would often start conversations by
44 trying to place her. Sonia tells me that they would usually begin by asking for her
45 surnames and the name of her village. From there, people would ask her where she
46 lived in the village and whether she knew, or was related to, other people that they
47 knew, including the elite families in Porcuna who were well-known in the region.
48 These discussions developed into how well she knew these people and how close
49 she was to them. Within a couple of minutes her social and familial networks had
50 been probed and her proximity, or allegiances, to certain people ascertained. Sonia,
51 likewise, initiated or reciprocated these conversations. These discussions also
52 covered the relative economic (in)security of each of the discussants.
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3 Sonia's approach to conversation differed from my technique. She wanted to share
4 experiences and I wanted to do research. If a conversation was on the intensities of
5 conflict or violence, she would ask sincere, spontaneous, empathetic questions that I
6 could only ask in a far more investigative manner. Occasionally, Sonia was there as I
7 talked to participants, especially if we were eating or drinking. If the conversation
8 turned to historical violence, details of incidents and characters involved would be
9 forthcoming and she might say something like, 'Do you remember what we went
10 through?' Meanwhile, I would be asking, 'How did it feel when the violence was very
11 intense? Can you tell me what happened?' If she was not there when this subject
12 was discussed, I got very little information other than, 'the violence was bad', or
13 descriptions of how they suffered and the names of well-known armed groups.
14 Without Sonia, the stories I heard had less depth and distanced the violence from
15 the intimacies of political, economic and personal conflicts in village life.

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18 Unencumbered by my family in Porcuna, who knew more about where I was than I
19 did, my research confidence grew in Andujar. I slowly developed a basic
20 understanding of local history and got to know many people. I felt a part of village
21 life. Moreover, in Andujar, there were fewer stories about recent murders and
22 violence than in Porcuna – the place felt safe.

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25 That changed when there was a nasty murder in Andujar that was characterised by
26 gory theatrics, the body reconfigured with a child's toys. My research theatrics,
27 featuring an anthropologist with a notebook, were about to be confronted.

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30 A young child witnesses her mother's death and stays with the corpse throughout
31 the night until neighbours break open the unanswered door in the morning. Word
32 goes around. We hear about it a few days later from someone who works for the
33 coffee cooperative where I conduct research. By then, there had been a second
34 murder, also grisly. 'It's ok,' I reassure Sonia, 'I know many people here and can find
35 out more. People will tell me if things are getting dangerous.' She stares at me
36 angrily, not for the first time, but this look has fear in it too. 'I don't know what is
37 happening,' she says. So I reassure her again. The benefit of ethnographic research
38 is that over time an anthropologist gets to know people quite well. I am sure they will
39 tell me more. Sonia has lived through a history of conflict and murders like this one

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3 but in Andujar she is unsettled by her lack of knowledge about the people who live
4 here. 'In my village,' she says, 'I know who to talk to and who to trust. I would know
5 about this as soon as everyone else did. I would know what happened, the family of
6 the person who was killed, their life, who might have killed them and why. Here we
7 don't know anything. We don't know who we are here or who anyone else is. I don't
8 think *you* should go out for now.'

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15 'We don't know who we are here or who anyone else is' - an echo of positioned
16 ethnography; less notional and more threatening. Our security was embedded in the
17 stories we told and the stories told about us: we were who we knew. In the
18 disjuncture between her position of security in Porcuna and uncertainty in Andujar,
19 Sonia could see our presence was ambiguous and our new contacts indeterminable.
20 Unlike me, Sonia's experience of conflict and violence told her that this type of
21 uncertainty increased our vulnerability. Despite all the conversations with local
22 people, she was experiencing outsider life – a disassociation from her familial and
23 social relations. Her insider knowledge understood those associations were the very
24 means of navigating violence, they were the way for Sonia to understand more about
25 her position relative to the positioning force of violence.

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31 Looking back at my field notes, academic reports on the silences of violence and
32 local people not knowing who was who (e.g. Green, 1994) appeared less robust. I
33 thought I saw my own reflection in other inquiring researchers with their alterity
34 visible, communicable, and purposeful. Where trust is indeterminable, the person
35 searching for information that they do not really need to know, is someone to treat
36 with caution (see also Salamone, 1977). So the researcher takes what their
37 companions give, or what they can safely report, even if it is silence. Thus, the
38 scholarly process to understand the silence more than the private conversations of
39 everyday life begins (Green, 1996; Green, 2011).

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45 Private conversations, which are harder to access, were more important to Sonia
46 than the public secret that discretely went round Andujar after the murder. The public
47 secret, 'that which is known but cannot be articulated...situations in which people
48 dared not state the obvious' (Taussig, 1999: 5-6), or 'the concealment of revelation'
49 (Jones, 2014: 55), suggested that the murder resulted from the victim's earlier
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3 affiliations to gangs – apparently old disputes were being violently settled. For Sonia,
4 however, the real question was not whether police, military, paramilitary, guerrilla,
5 gang or thief committed the murder. That information described the wider political
6 context of insecurity, but it was not personal. The companions of the victim and
7 perpetrator were part of the story too; their family, friends and business relations,
8 who they had sex with, where they shopped, where they ate and drank, how much
9 money they had, who they owed money to and why. Who were our neighbours now
10 and were these people also part of the murder story? Just like the introductory
11 conversations when Sonia met people in Andujar for the first time, story-telling about
12 the murder allowed Sonia to position the violence in relation to her life and that of her
13 companions too. The connections, reputations and locations of family, friends,
14 employers, and business and political associates just outside the immediacy of the
15 murder, were companions to the murder.
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19 Security is positional to violence. Sonia and much of her family had a position that, to
20 various degrees, protected them in Porcuna, depending who they were, what was
21 going on, the stories they told and the stories told about them. The communication of
22 violence is not just through the performance of violence but through public and
23 private discussions about that performance (Caldeira, 2001; González, 2011). These
24 discussions, often referred to as rumours and gossip, are informed by the
25 performance of violence and also shape the violence that is performed, for example,
26 in colonial plantations (Taussig, 1987), paramilitary and police death squads
27 (Taussig, 2003) and witchcraft (Stewart and Strathern, 2004); the phenomena has
28 been studied quantitatively too (Greenhill and Oppenheim, 2017). These
29 conversations are also bound to all the other conversations about family, and other
30 social, economic and political affiliations. The history of conflict is an enacted and
31 discussed history of inter-related positions (Eltringham, 2004). 'Security is thus
32 inherently performative, discursive and intersubjective' (Goldstein, 2010a: 127).
33 Although the fluidity of position denies anyone absolute 'neutrality', the gravity of
34 particular configurations and intensities of violence, in particular locations at
35 particular times, draws some in more than others.
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39 My companions in the field were providing key information for the research and I
40 could not ignore the contribution.^{□2} I noted how conversations connected to a history
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3 of violence, thus connecting people and places to ongoing threats, risks and
4 performances of violence. The conversations acted as a cartography of violence for
5 local people to navigate based upon their direct and indirect relations with violent
6 actors and places. Living with impunity and economic insecurity provides space for
7 other systems of security and welfare including privatised security as vigilantes,
8 lynching, gangs or personal retribution (Starn, 1992; Goldstein, 2010b). In these
9 circumstances, how people identify other people is not just through class, gender,
10 age, ethnic or sexual relations, but also through their potential for violence, either
11 directly or by association. In a location with a long history of conflict and violence,
12 these violent relations are matters of everyday conversation and social position in
13 the village.
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24 Just as there are companions to the researcher, so too, those companions have
25 companions of their own. Family companions are not confined to an apartment in a
26 village, their interactive presence is structured by the family's social associations,
27 connections, separations and fractures that assimilates a wider family and
28 acquaintances into the field site family. A family who have lived in a conflict might not
29 be viewed as 'central' to the conflict (Hoffman and Tarawalley Jr, 2014) if conflict is
30 interpreted as synchronic violent *events*, unless the family were direct victims or
31 perpetrators. Nevertheless, like the rest of the population, family are central if conflict
32 (and violence) are viewed as the developing social and economic relations of
33 diachronic violence in a locality. In the latter case, they are always associated with
34 violence, my family have a violent position that shifts with time and circumstance.
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45 The ambiguity, created by how far I am willing or need to perform or parade my
46 family for the reader, serves another purpose. Within every extended family and the
47 wider possibilities of kinship (Carsten, 2004) there are murky spaces. Who did
48 people meet when they met me and my family, and who were they to us? Who we
49 are and who we encounter – and how we position each other – takes place within
50 incomplete and fluid local histories of social knowledge. In those histories, violence is
51 one thread, or more specifically, violence is thread formed by the coming together of
52 many other threads.
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In daily encounters, this precarious balance between security and insecurity gives

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3 meaning to violence, identity and the relative strengths of social relationships. In
4 Andujar, my family were less known to the villagers and the villagers were less
5 known to us. We tried to position them – Sonia more than me. She was told about
6 the known thieves, knife stabbers, paedophiles, paramilitaries, wife batterers, and
7 murderers (and their associates). Where did we fit into this story-telling of violence
8 performed in partnership with enactments of violence? Were we people to be
9 threatened, people who presented a threat or somewhere in between? To different
10 people we might have appeared differently. My refusal to disclose much information
11 about my family is a silence for the reader but not for those who know my family.
12 Indeed, for those who grew up in Porcuna, the hidden corners of my family are small
13 and few. If the reader is not confident that they can grasp the position of my family to
14 violent actors in relationship to research participants, and therefore to our violent
15 threat, then they share that uncertainty with many of the villagers that we met.
16 Similarly, as seen by Sonia's fear after the murder in Andujar, a village different to
17 her own, we too had difficulty positioning many of them.

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Violence, and the research encounter of violence, is relational. Whether we live in
the coffee hills of Colombia or the universities of the UK, we are connected to those
people who would perform violence on us, or for us. The priority of focusing on those
violent relationships depends on the proximity and weight of our associations (as
influential, protective or threatening) to differing intensities, practices and discussions
of violence in the places we inhabit at any one time. Like those around them, the
relations that a researcher can or wants to perceive with their companions, positions
them in a forcefully positioning violence, and so informs their (research)
interpretation of what violence might be.

Accompanied Fieldwork and the Research of Violence

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Gupta and Ferguson (1997) undermined the pretences and power plays of isolated
field researchers in isolated fields almost 20 years ago, yet the continuing argument
against the illusion of 'unambiguous individuality in fieldwork' (Jenkins, 2015: 2)
suggests that the idea of the lone fieldworker remains an anthropological norm.
Scholars think that authors should respond to companions in the field as a situation
that requires disclosure about the 'realities of access' (Hoffman and Tarawalley Jr,

2014: 302) whether writing about family companions (Korpela et al., 2016) or defining 'the field' (Gupta, 2014). The transparent method is also recommended for research on violence (Hoffman and Tarawalley Jr, 2014; Middleton and Cons, 2014). The reasoning, which derives from an earlier reflexive turn, is understandable (Davies, 2008). People who we research, locate our research companions 'for better or for worse, in a particular cultural context' (Seiler Gilmore, 1998: 41), which further positions the accompanied researcher who writes ethnography.

For many years, accompanied fieldwork has been identified as a widespread practice that is largely unacknowledged in publications (Fontana, 1975; Gottlieb, 1995; Flinn et al., 1998; Cupples and Kindon, 2003). Where the presence of research companions is analysed, the discussion is divided into two overlapping groups: companions as family, or intimate partners, (with or without children) and companions as research assistants (local to the field or from another region). My family share some but not all of the qualities discussed.

Family companions appear in a range of situations. A husband (university teacher, non-anthropologist) complains that his anthropologist wife forces him to participate in her fieldwork (Simon, 1977). Research companions as intimate partners are contrasted to research collaborators trying to finish a project (Kennedy, 1995). Parents and children work as long-term research assistants with a visiting anthropologist (Gold et al., 2014). In a collection of different accounts of 'Fieldwork and Families' (Flinn et al., 1998) almost every researcher is accompanied by an academic adult companion. More recent reflections on accompanied fieldwork once again include research-qualified spouses (Lunn and Moscuzza, 2014) or husband and wife researchers who engage their children as research assistants with all writing an article together as a family activity (Starrs et al., 2001). Most families in this literature do not come from the field area. Family life, or accompanied field life, is not so far from scholarly life. The preponderance of shared professional and cultural backgrounds, within these accounts of companion fieldwork, is a view of mostly white, educated, academic, middle-class families.

Middleton's (2014) observations about local research assistants applies to family companions too. They are neither inside nor outside the field and their relationships

with the researcher and the local community change over time, shaping personal and political positions. There is a temporal dynamic between researcher, assistant and community that produces the research and leaves everyone's lives somewhat altered. Following Latour & Venn (2002), companions appear as intermediaries but are actually mediators. Companions inform ethnography, even if their contribution is later obscured.

The researcher of violence accompanied by family has not attracted much attention. The 'insider' local research assistant while helping the visiting researcher navigate the particular dangers of a conflict (Smyth and Robinson, 2001) is, nevertheless, entangled within the history of conflict undergoing study. Moreover, their identity and social position influences the choice of field sites, how interviewees are found and how local public perception of the research is managed, so shaping the stories a researcher hears or does not hear (Jenkins, 2015: 13). The companion from a local area provides insight into the violence but, because their position is also a critical element in the development of the researcher's point of view, some suggest their role needs to be made explicit.

'...we have to collaborate with people who are central to the violent events we seek to understand. There are elements of the field assistant relationship that cannot, and certainly should not, be easily written out of the resulting text. They are simply too central to what it means to do ethnography through frontline collaborations.' (Hoffman and Tarawalley Jr, 2014: 307)

Yet, deciphering the position of research companions is complicated. Violence requires daily interpretation by those living amongst the violence. Identity and social position are layered, inter-sectional and fluid qualities emerging from individual and local histories of kinship, labour, and land-ownership or mediated through performances and other locally interpreted relations that form discourses on gender, class, sexuality, ethnicity and personality. These multifarious dimensions of being, with a complexity of interwoven public and private personal histories, must be considered in the context of conflict that drives social polarisation using techniques of violent differentiation based on more essentialist interpretations of being (see Appadurai, 1998). The superficial rationale behind conflict can complement or

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3 compete with the multiple nuances of public biographies.
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7 How we frame companions in a conflict is tied to how we frame conflict and field-
8 based discussions about conflict. Jenkins recognises this when she explains that the
9 ethnic identity of her assistant, which provides them with supposed neutrality,
10 intersects with his previous participation in political violence. It is a convenient
11 convention of reflexive writing to be able to name the influence of our companions in
12 the field, and, in doing so, position their influence - but these pictures remain
13 incomplete. What Jenkins does not record, because she was not privy to those
14 conversations, is what people said about her assistant that they did not want her to
15 hear – the key questions like who he was, who he knew, what else he had done.
16 Ideas about researcher (and research companion) ‘neutrality’ (Jenkins, 2015: 9)
17 derive from a view of conflict as a contained event within social interaction rather
18 than as a conflict inter-woven into multifarious details of everyday history.
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29 There are limits to understanding how a companion’s involvement is seen by those
30 in the local community. When family are companions in the research of violence, the
31 researcher must reconcile their own view of their family with how they are told that
32 the local community sees the family. By excluding my family companions from the
33 research, I mistakenly thought I had excluded their involvement and views about
34 them from the investigative process.
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41 Literature on companion fieldwork, mostly omits those who do not act as assistants
42 or academic interlocutors and so reduces them to non-influential positions. The one
43 notable exception is observations about children. Flinn and Marshall (1998) describe
44 how children as research companions: pragmatically create access through their
45 friendships and activities; provide characters in methodological discussions on
46 safety, work-family balance and parental responsibilities; and analytically generate
47 encounters for later insight, especially with studies on gender and kinship. On the
48 whole, children do not consciously gather ‘research’ information, discuss
49 methodological and theoretical possibilities or, for the most part, elect to be involved
50 in the parent’s work. Their contribution is marked, yet most of them remain non-
51 research participants in local life, they are in the field to live non-research lives. The
52 influence of close companions who do not practice research is significant,
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unintentionally they move into the researcher’s frame.

Relations Between Me, My Family, and Readers

A key problem in the research of violence is writing about violence. When research insights emerge through family, then the balance between reflexivity and anonymity requires careful mediation. Researchers are encouraged to disclose the ‘appropriate or fruitful’ (Hammersley and Atkinson, 1995: 91), but what might be *fruitful* for the argument and the reader could be *inappropriate* to divulge. During writing, companions that have been in the ethnographic shadows - like readers and academic institutions - become more prominent. The time before and after the research trip (Roberts and Sanders, 2005) and the ‘career trajectories and etiquette’ of academic life (Sangren, 2007: 13) are tied into the writing process.

The inclusion of family companions in research can lead to questions or (incorrect) assumptions that, whether understandable or not, remain intrusive. Obscuring key information that situates the perspective of field inquiry undermines broad claims to the veracity and integrity of ethnography. Additionally, the intimacies and loyalties towards family could lead a researcher to paint their companions in a good light rather report on more troubling moral ambiguities. When my research was under review, one reviewer extrapolated from missing information that my family were up to their necks in violent business and requested that I ‘spill the beans’. The reviewer or reader, mindful of how their own ethnographic experience informs their reading of other ethnographies, might be tempted to play the anthropological detective: their educated imagination attempts to solve the narrative lacunae like clues left behind at an ethnographic crime scene.

How far should the lens of inquiry, that is now public, be turned onto the private body? In the realm of reflexivity and inclusion of one’s companions within ethnographic production, and in order to reveal understanding from the field experience that would otherwise have been obscured, the author is asked to expose themselves and their companions (or, at least, make epistemological flirtations around those acts of exposure) in order to satisfy their audience’s expectations. In

the case of review for publication, or examination, perhaps inevitably, there is coercion on the author to reveal.

Companions fundamentally shape ethnographic research but family companions are unprotected from exposure using standard research strategies. In the research of violence, these problems of anonymity carry security risks too. As a named author, characterisation using pseudonyms fails to provide my family with the same degree of anonymity as it would with other research participants; a predicament that also encompasses extended family - the companions of my companions (Hopkins, 1993). The exposure of Scheper-Hughes' study of 'Ballybran', in Ireland, demonstrates how easily the anonymised can be recognised, with detrimental consequences (Scheper-Hughes, 2001; Kane, 1982; Viney, 1980; Viney, 1983).

If we want to grasp insight while avoiding intrusion, the flexibility and innovation recommended in the research and writing of violence (Kovats-Bernat, 2002: 210) can be extended to our reading of violence. 'Negotiating and managing privacy is an issue for both researcher and researched' (Coffey, 1999: 74), but it is also an issue for the reader. Demands to know the shadows of text are usually satisfied by a writer's technical capacity to deflect, disguise or deconstruct the shadows. When these techniques are not deployed, or are unsuccessful, the reader faces an ethical question - what can they appropriately ask their author to divulge?

There is an assumption that the hidden companions of anthropologists, who are confined to the acknowledgements of articles and books, are simply ignored or undervalued in publication. Another possibility is that those authors and their companions protected themselves from the readers. Concealment and revelation are present in the writing process as well as in ethnography (Jones, 2014). Anonymising family as someone else's family, or omitting them altogether, would limit damage. The companion family is better protected lost in the fiction than left in the open with only the sword of truth. This ethical stance prioritises the protection of researcher and family, and so de-prioritises the research participants, especially those marginalised, never met or portrayed negatively as a consequence of this (hidden) family first strategy. Whatever strategy an author adopts to protect their companions,

they need to take care to clean up the clues before the anthropological detectives arrive, otherwise, the author risks an interrogation to confess what they do not want to know.

I never found out much detail about my own family's proximities and connections to violent actors. After much consideration, I decided that I would not actively investigate these questions but would take note if the subject came up. Like many families, once kinship and relationships are extended beyond the nuclear model, my family include, in the broadest of terms, the rich and poor, the brutal and the suffering, and much between.

Prolonged research 'back home' can be an attractive proposition for families with migrant backgrounds, but the research project persists in family long after the fieldwork is over. When researchers conduct investigations in places where their families go home to be with family and friends, ethnography and family are affected. When the study concerns violence, like other sensitive topics, the position of companions intensifies practical and ethical problems. Latour's description of the sociologist who is 'sent away in order to come back' is ill-fitting (1987: 211). Family life, whether 'home' or 'away', is always 'home'.

Should family, or other companions, not willing to be represented in text be asked to leave the researcher's company? The under-reporting of companions, alongside the gradual realisation during recent decades that we are not alone in the field, has allowed the problem to fester. If researchers are expected to be transparent and include their companions in text, are the only families that can go to the field the ones ready to engage in a full investigative and reflexive exercise? For migrant returners, especially non-academics, that might be a task they want to avoid, especially when violent relations abound. The authoritarian overtones of this return to Malinowski-styled, orphan-on-an-island ethnography are only avoidable if readers reflect on the ethics of their own expectations of text, so allowing authors to engage more deeply with what their more intimate companions tell them.

[From Violent Relations to Relational Anthropology](#)

The tension between the private and public, or concealment and revelation, is an ethnographic trope that applies to the ethnographer as much as to those the ethnographer studies. Perhaps readers are uncertain about the violent background of my companions yet, can see how these violent relations weave into what and who we are, at home and in the field. If so, then they share the perspective of the coffee farmers that I studied, who told me they were uncertain about the violent background of their new neighbours, worried by others who they did know and glad to have a positive association with some more powerful folk in the village. As one farmer told me, 'Until now, I have been lucky with my neighbours'.

Whether university-based researchers or people living with higher levels of violence in a coffee farming village, we position ourselves in relation to our companions, who do the same with us. Companions, of various forms, affect each other in a fluid history while telling stories, by various means, of their inter-affectiveness. From the toys in a murder to aunts in a village, each says something about how they connect and shape their companions, who in turn are shaping them. In a relational methodology, the relations that inform ethnography derive from a researcher's actual companions rather than those companions that they seek to study but sometimes spend less time with.

In order to gain greater relational insights, ethnographers need to listen to where they are rather than where they want to be, although this strategy is unlikely to satisfy those readers or funders who expect something more determined from their social sciences. And so, like coffee farmers positioned by companions that (in)form their (in)security, the author-ethnographer is further positioned by institutional and academic companions that (in)form their (in)security. Whether coffee farmers or ethnographers, the stories we tell and the stories told about us within our companion relations provide opportunity into, and restriction from, other emerging relations. Violent relations abound.

The insights and problems of disclosure that derive from close companions cannot be confined to a human-centric view. As anthropological deliberation moves further from delimiting abstractions, such as structure, culture or society, it heads towards the emergence and practice of more fluid relations. For some, this includes relations

between human and non-human companions (Kirksey, 2014) including plants (Chudakova, 2017), or relations that emerge with different companions in a human body, for example, relations formed through neurobiology or hands (Toren, 2015).

Some readers might argue that family companions are a special methodological case who, unlike other companions, have undue influence on ethnography due to their intimacy. Yet, in relational anthropology, the possibility of intimate companionship and subsequent influence is multidirectional. Frameworks that de-centre the human makes it harder to determine who or what 'companions' are. A research assistant could be a dog called Torridon (Lane, 2015). Tensions between fruitful insight and appropriate disclosure continue to hold relevance because the ethics and reflections of reader-author relations reveal much about companions in the field.

At one level, a relational anthropology is yet another incarnation, or refinement, of Marx in which people make history but not in circumstances of their own making. People, bits of people, other species, affects, material beings, environments, times, spaces, virtual spaces, and transcendence make relations but not in relations of their own making. The frame of available circumstance has burgeoned along with the range of subjects in inter- and intra- subjective relations. We are not alone. On the contrary, we are surrounded.

The everyday is conceived as transient gatherings among a host of companions who, like ourselves, are shaped by the trajectory of their companions, and in which every companion emerges from a relational history. Scholars question how sticky these relations are and endow them with varying qualities of influence, stability, coherence, formation and commensurability. Whether named as cyborg (Haraway, 2013), partial (Strathern, 2005b), network (Latour, 2005), assemblage (Deleuze and Guattari, 1988), or post-human positioning (Braidotti, 2013), the common theme is a conceptual prioritisation around the discovery and interpretation of selected relations. To coin a phrase: 'Anthropologists do it with relations'. Emails that publicise a call for anthropological papers announce new companion relations almost every week. For example:

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5 'Our starting contention is that although bricks could be considered ordinary,
6 they tell complex stories about bodies, (in)justice, and power. Bricks matter;
7 their making and consumption having political, material and affective force as
8 a vibrant form of infrastructure which mediates and organises life.' (Brickell,
9 Parsons et al. 2018)
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15 Fieldwork, like other experience, is an immersion in a range of companions, from a
16 diary to a road, a wristwatch or a phone, or some bricks, the laughs over a beer or
17 the tears of loneliness – all are (in)formative but only provide research value when
18 the lens of positioned meaning is placed over them. The identification of companions
19 and their influences ultimately derives from how, in embodied and textual practice,
20 we conceptualise 'the field'. The field is formed and constrained by companions,
21 including the companionship of pre-packed research baggage. The sum of all
22 anthropologies could be brought to the field experience. Although my research
23 questions are meant to deal with one aspect of the field, the message from my
24 primary daily companion relations might have more power and relevance than
25 relations that I go to study.
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36 An ethnographer's research lens influences what companions a researcher sees and
37 what they see in their companions (and what companions might see in the
38 researcher). Strathern's (1987) displaced focus, to look at one thing in order to study
39 another in the periphery, can be an accidental effect of ethnography. Wherever the
40 researcher chooses to look, the field is ultimately self-revealing both as the
41 researcher's emerging vision (arguably a chimera) and as it reveals the position of
42 the researcher. If ethnography is formed through the body of the ethnographer then,
43 in a relational field, the ethnographer's companions form the relational field that the
44 ethnographer encounters. Therefore, a relational ethnographic field can only be
45 found in the relations of the researcher, as such it can neither be visited nor escaped
46 and is always on the move.
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56 As anthropology finds ever more companions to study, how will the anthropologist
57 identify and manage the intimacies of their immediate companions which are
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entangled in the research? The companions of fieldwork cannot be ignored even when they want to be excluded. Relations uncover relations (Strathern, 2005a). Like getting to know neighbours in Andujar, making relations from companions is a temporal and incomplete process of practiced positioning in which the intimacies of companions are positioned in conjunction with our own intimacies and interests.

The ethnographer who does not want to explore the shadows of their own intimacies will always be on the run from their own research. The focal point of research determined prior to fieldwork must eventually collide with the alterity of ethnographic encounter as new companions come aboard. The field is a fluid positioning: the field is our relationships with our (research) companions. Regardless of our research aims and travel tickets, our companions are our field. If anthropology is a relational pursuit, then anthropology is companion studies.

¹ All dates, locations and names have been altered in order to provide a degree of anonymity

² 'Ethical Guidelines for Good Research Practice' by the Association of Social Anthropologists regard consent 'after research is completed' as 'not meaningful', while consent 'in research' is considered as a process 'not a one off event, and may require renegotiation over time' (<https://www.theasa.org/ethics.shtml> accessed 5/12/2017). On this basis, I renegotiated the terms of engagement with my family in order to get their consent.

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